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THE INTERPUPILLARY DISTANCE AS THE CORNERSTONE OF THE OPTOMETRIC EXAMINATION*

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Optometry has wasted years arguing about the practicability and impracticability of this or that method of measuring the interpupillary distance and has failed withal to arrive at any acceptable standard and, from the nature of the case, may expect nothing but failure if she continue along these old lines.

Great stress has been laid upon the means of obtaining the most precise measurement between two imaginary points with no agreement as to which of three or four pairs of imaginary points shall be subject to measurement.

The discussions have revolved about the questions—shall we measure from the center of one pupil to the center of the other, from the inner edge of one to the outer edge of the other, from the inner edge of one cornea to the outer edge of the other, or shall we measure from the center of rotation of one eye to that of the other? Then, as though this were not sufficient ground to argue over, we have included the further questions—shall we use in measuring whatever we do measure a common rule or a Maddox localizer as described by Phillips[1]; a Laurance[2] Orthops rule or the Landolt[3] chiasmometer; the Stevens rule or the Frankel face measure; the interpupillary gauge of Busch[4], Oppenheimer[4], Jung[4], Horstmann[4] or Hertel[4] or any one of a host of similar devices which, however simple or complex and however accurate in themselves, cannot hope to solve the problem so long as the points to be measured are indeterminate.

It would seem, then, that to settle upon two points, any two definite points, would put us in a fair way toward the settlement of the issue but with this we cannot agree, after careful study of the factors involved. The factors studied are:

(1) Whatever attention in present-day practice is accorded the interpupillary distance is relegated to the end of the examination.

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(2) Whether applied by the optometrist himself or by his assistant the measurement is related solely to the frames and lenses to be made up for the patient's use.

(3) There exists no uniformity in either making the measurement or in applying it, each optometrist being a law unto himself.

(4) There is need for a standard or uniform procedure and for one that includes more than the accurate measurement of the interpupillary distance, per se.

(5) The confusion that has grown up around the methods and means of making the measurement has resulted in hiding the basic significance of the relation of the measurement to every other step in the examination thereby introducing a series of collateral errors more serious than any error likely to be made in the measurement itself.

From consideration of these factors we arrive at what appears to be a sensible solution of the whole problem embodying the following:

(1) The measurement of the interpupillary distance shall be the first step in the routine optometric examination.

(2) A reasonable tolerance shall be allowable.

(3) A fixed relationship shall be maintained between this interpupillary measurement of tolerable accuracy and every piece of apparatus used subsequently throughout the examination and so on through to the finished glasses.

The advantage of such a procedure must be obvious to anyone who sees that by following it we eliminate most if not all the danger that might arise from an inaccurate estimation of the interpupillary distance and make any reasonably accurate measurement safe provided this measurement be used at every step in the examination so that the trial lenses, the optometer lenses, the phorometer lenses as well as the patient's final correcting lenses are set definitely to correspond to the original interpupillary distance estimate.

Nothing radical is called for to evolve a little cosmos out of the present chaos. It surely is not radical, is it, to take the interpupillary measurement at the beginning instead of at the end of the examination, to be satisfied with a reasonably accurate examination room method which is available rather than to seek vainly a method so precise as to not survive outside the laboratory, and to base every test and every prescription upon this reasonably accurate measurement in such a way as to automatically compensate any inherent inaccuracy by the strict adherence to some one common distance between the centers of the lenses used in the tests and those used in the prescription?

Unless we have been grossly misinformed and pitiably unobservant we are correct in stating that the usual procedure anywhere and everywhere, except of course in Academy circles, is somewhat like this:

A trial frame is placed upon the patient and its cells so set that the eyes come in about their centers, few, if any, using the cross-line disks; retinoscopy and subjective tests completed; muscle balance investigated;

the cells brought a little closer to each other and the reading addition estimated. Or the lens openings of an optometer of one form or another are set about right and similar tests conducted. Or from the trial frame or optometer the patient is transferred to some form of phorometer whose cells in turn are set at about the correct interpupillary distance.

But what is the interpupillary distance? Well, we will go outside and take facial measurements and get there what each practitioner considers his very accurate measurement of the interpupillary distance with that special untested but of course very accurate celluloid rule obtained at the banner convention in the Summer of '98, the end lines of which are pretty well worn off but whose zero line we recall begins along about here somewhere—it was very clear in '98 so we remember quite well where it *was*!

But what are we to do with this delicately or, shall we say, deliciously precise facial measurement obtained so cleverly at the tail-end of the examination while the interpupillary distances of trial frame, optometer, and phorometer have run wild throughout the entire examination?

We can prove, as can most of you without traveling far beyond your own offices, that examinations have been made with the phorometer openings set at 64 millimeters, the optometer at 58 millimeters, the trial frame at 62 millimeters while the true interpupillary distance turned out to be 60 millimeters, and frames built accordingly.

In spite of utmost accuracy in the interpupillary measurements obtained at the end of the examination the possibility of error cannot be eliminated so long as uniform setting is not insisted upon throughout the entire procedure.

To take the most incorrect figure of those just mentioned, 64 millimeters, and use it consistently in the tests as well as in the glasses prescribed will inject less error than is injected by the toleration of varying interpupillary distances between the various sets of test lenses used and the glasses prescribed.

But if we can obtain a reasonably accurate measurement with an interpupillary gauge after the optometric tests are completed with errors generously interspersed why cannot we obtain that same measurement at the beginning so that we may maintain it to the end?

In order to grasp the full significance of this point let us apply it to a few of both the usual and the unusual cases encountered in regular practice.

For example, take the case requiring O. U. +2.50, with an interpupillary distance of 60 millimeters. If in such a case we use the phorometer with its cells set at 64 millimeters we introduce an error of 1Δ base out; from that go to the optometer set at 58 millimeters and we shift the error to 0.5Δ base in or a total of 1.5Δ between these two tests; now go to the trial frame set at 62 millimeters and our error is 0.5Δ base out. Now, if the convergence can affect the accommodation, here is something to ponder carefully. But to drive the point home suppose the case required O. U. +5.00. In such a case the errors would be doubled and

run as high as 3Δ and in a myopic case of —10.00, call it usual or unusual as you wish, the prismatic error would run as high as 6Δ between two of the instruments most frequently used.

So here we have examples of the practitioner who would be horrified at dreaming of deliberately prescribing such a thing as a 6Δ prism but who blithely permits these values to zigzag through his tests and bury themselves (and his good name sometimes) in his finely centered 60 millimeter frame.

Exaggerated? We hope so. But an honest audit of our present procedure may be depended upon to afford many of us a thrill.

In its very simplicity, its very obviousness, lies the difficulty of getting this point taken seriously. We all assume we take care of such fundamental, vital principles and cannot believe we could overlook such a basic factor but nonetheless as a matter of fact the point is accepted in theory but rejected in practice.

We have said nothing about vertical variations between the test and the correcting lenses which introduce another serious disturbance but this is another phase of the subject and has had its remedy clearly outlined by Ryer[6]. Nor in this connection do we know what to say about the optometrist who shouts that prisms are poison while he naively or diabolically (according to viewpoint) permits all sorts of prism values to run amuck in his tests where they slyly modify his findings while he smugly assumes he is not using the despised poisons his foolish confreres so recklessly administer.

Who, now, can doubt that many of the conflicting data which so obstinately refuse to be reconciled in practice are the direct result of this lack of uniform centering from start to finish and who can look for anything but confusion and error if this procedure of uniform centering be not established.

How many can answer from definite knowledge and with truth in the affirmative the following pertinent questions:

(1) From careful, trustworthy personal tests do you know that each lens of your trial set is accurately centered?

(2) That each lens in your phorometer is accurately centered?

(3) That each lens in your optometer is accurately centered?

(4) That each cylindrical axis is accurately designated?

(5) That each prism base-apex line is accurately designated?

(6) That the interpupillary distance calibration is accurate on your trial frame, optometer, phorometer and interpupillary gauge?

Somehow these questions recall the story of the colored preacher who hesitated, he announced one Sunday morning, to present the title of his sermon until he ascertained how many present had read the XXIX chapter of Matthew; the hand of every person in the congregation went up; upon which the preacher drew a sigh of relief and calmly stated "My sermon is going to be on 'Liars'—there is no XXIX chapter of Matthew."

A year ago we would have innocently raised our hand but today we know that our instruments are far from being free from errors. We tested each and every lens of trial set and optometer and found not a few decentered lenses in each and you may be sure that there are such in yours, of whatever make it may be, and we may be sure furthermore that we cannot procure strictly precision lenses in trial case or optometer because we have never shown the least willingness to pay for precision work.

Our problem was, then, how to apply to best advantage what we had, which while it embodied inaccuracies was the best procurable. It may seem strange to you that we deliberately contemplated compromise on this point by using agents we knew to be inaccurate but it will seem more strange to us if you continue to use these same inaccurate agencies without adopting the means hereby offered of neutralizing or compensating their inherent inaccuracies.

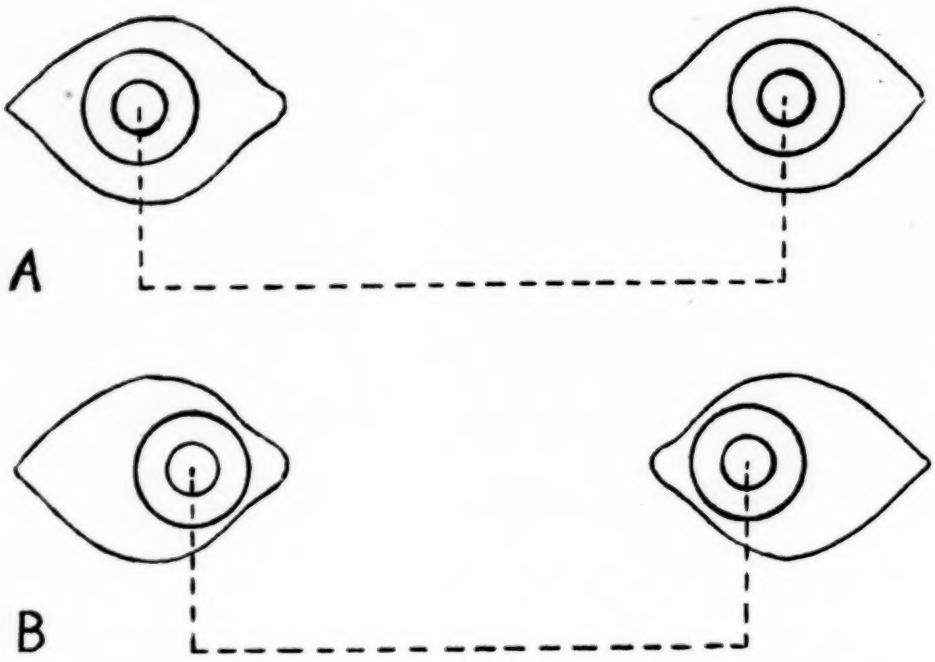
Up to this time the issue seems to have been dodged; we are meeting it and no longer hiding behind the smoke screen of angle alpha, center of rotation, visual line, optic axis, line of fixation and so on but offering a definite remedy for one of the most fundamental factors in the whole optometric examination—the co-ordination of the interpupillary distance with all tests and their resultant prescription.

Possessing a highly corrected trial set we clung to the notion that it was superior to the optometer's battery of lenses. We have reason for questioning the wisdom of that course. True, we found rather more error in the centering of the optometer lenses than in those of the trial set but the optometer lenses come before the eye always in exactly the same relative position, that is, if the center of one lens or combination of lenses is displaced slightly it will come always before the eye with its center thus similarly displaced while in the case of a decentered trial lens that center may be displaced in at one time and out or up or down at another depending upon how it happens to be dropped into its cell in the trial frame.

Furthermore, the axes of the optometer cylinders will come to the same position before the eye with a far higher percentage of accuracy than the axes of the trial set cylinders can be set by hand and again the prisms of the optometer and phorometer afford infinitely more accuracy, with all their faults, than the hand-set prisms of any trial set.

Hence as the manufacturers, on an economic basis, insist they are working within reasonable tolerance limits it is up to us to use to best advantage the only instruments we can procure and the procedure we are here advocating partly compensates the errors of centering that do exist and avoids introducing still greater ones; and, even were it possible to obtain absolutely accurately centered lenses in every instrument used, it would still be necessary to adopt this procedure or its equivalent to avoid reintroducing the errors of decentration we now wish eliminated.

Is it not evident that a pair of perfect +5.00, perfectly centered, used in a phorometer set at 64 millimeters interpupillary distance will act differently than an equally perfect pair of +5.00, perfectly centered, used in an optometer set at 58 millimeters and that still more difficulty will

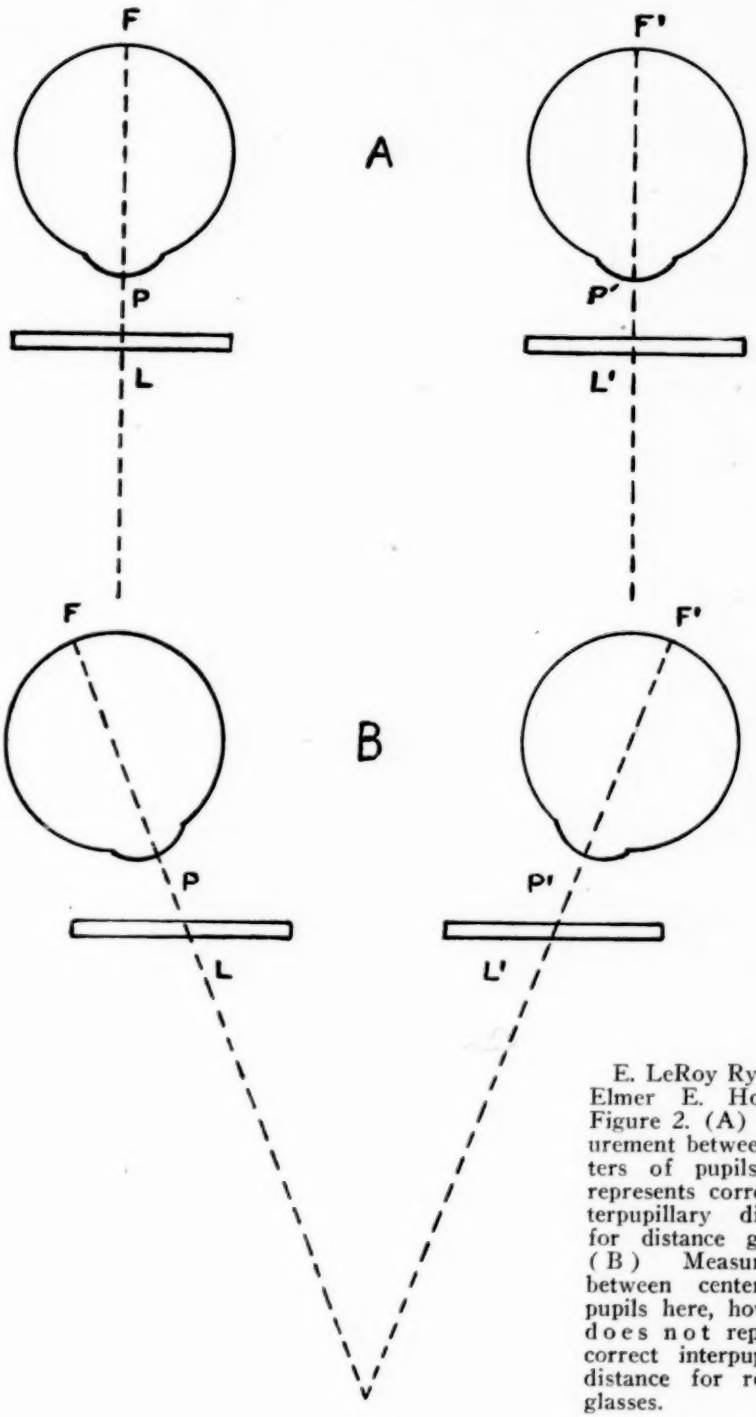


E. LeRoy Ryer and Elmer E. Hotaling. Figure 1. (A) Measurement between centers here represents correct interpupillary distance for infinity. (B) Measurement between centers here during convergence toward reading point does *not* represent correct interpupillary distance for reading.

arise when that pair of perfect +5.00, perfectly centered, is translated into a pair of glasses for a patient who has a perfectly fitting frame of 60 millimeters interpupillary distance?

If all instruments as well as the final glasses were set at 58, at 60, at 62, or at 64 millimeters—that is, at some *one* common distance at every step in the procedure—the error caused by any decentration in the first measurement will be compensated as the tests proceed just as surely as proper procedure would ultimately compensate an artificial imbalance caused by placing deliberately a 3Δ prism, for instance, before the eyes being tested. It would result, theoretically it must result, in our ending with a 3Δ base the opposite way in our final correction; but proper technique would compensate it whereas at present we introduce into our glasses any and sundry prism value, frequently as high as 6Δ, with complete abandon, which was not present in our tests and vice versa.

Thus if this procedure be adopted of simply getting a reasonably accurate interpupillary distance measurement as the first step in the examination and thereafter setting every pair of lenses used for test and for wear at that same distance we may forget all about the dispute as to



E. LeRoy Ryer and Elmer E. Hotaling. Figure 2. (A) Measurement between centers of pupils here represents correct interpupillary distance for distance glasses. (B) Measurement between centers of pupils here, however, does not represent correct interpupillary distance for reading glasses.

whether the measurement should be made between the centers of the pupils, the centers of the cornea, the centers of rotation, the right and left edges respectively of the pupils or of the cornea and we may cease also debating whether we should center the lenses for the optical axes, the visual lines or the lines of fixation. We may ignore, if we ever recognized, the fact that synechia may cause an error in the measurement when a light or other form of stimulus causes the pupil to react more in one direction than in another. And we may dismiss the warning advanced by Laurance[5] against measuring from the right or left edge of one pupil to the right or left edge of the other when one pupil is larger than the other in which case an error will result equal to half the difference in the diameters; nor need we guard against the error that would result if dilation or contraction take place during the process of measuring perfectly normal pupils.

May we interject at this point a thought which though only indirectly connected with our subject is of such vital import as to warrant a moment's attention: If, in taking interpupillary measurements, one eye is found to be considerably higher or protruding more than its mate suspect tumor, orbital and possibly encephalic, and keep suspecting it until evidence is adduced of its non-existence.

Now we are ready to consider the steps necessary to the establishment of this proposed procedure upon a practical, usable basis.

Well, if we are to measure something we must have something to measure and something with which to measure. After considering all factors we conclude that of all the pairs of points whose distance apart we are to measure as the interpupillary distance the pair conducive to greatest accuracy are those two points representing the center of each pupil.

The question may arise as to the probability of accuracy in thus locating two imaginary points and, even to the point of tiresomeness, we must reiterate that the necessity for extreme and therefore unattainable precision is the very thing we may avoid by adopting the proposed procedure of getting a tolerably accurate estimate at the beginning and adhering to it throughout the tests and prescription.

Of all forms of interpupillary measuring methods we prefer that of measuring the distance between centers while each eye alternately assumes its primary position. This may be done either with one of the more elaborate and relatively expensive gauges or with the simple and inexpensive slide-rule gauge shown in Figures 3, 4, 5.

With ordinary care dependable results may be assured by getting the distance from the center of the bridge of the nose to the center of each eye alternately as each eye, respectively, fixates some point straight ahead. The more elaborate gauges have fixation lines to be looked at while with the simpler slide-rule gauge the optometrist places himself squarely in front of his patient and presents his own eyes alternately as fixation points.

Perhaps there is no better place than this to touch upon an interesting and important principle that is not generally appreciated. It assumes that with the patient's eyes alternately fixating the equivalent of an infinitely

E. LeRoy Ryer and Elmer E. Hotaling. Figure 3. Proposed interpupillary rule when not in use, both shutter slides open.

E. LeRoy Ryer and Elmer E. Hotaling. Figure 4. Proposed interpupillary rule ready for measuring right eye, shutter slide shutting off left eye.

E. LeRoy Ryer and Elmer E. Hotaling. Figure 5. Proposed interpupillary rule ready for measuring left eye, shutter slide shutting off right eye, right eye measurement still intact.

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distant point we may obtain an accurate distance interpupillary measurement if we sight, also alternately, across the scale with that scale just clearing the lashes; but it assumes further, and this is the part not so well understood, that the reading interpupillary distance can be calculated more accurately from this distance measurement than it can be measured directly. Let us see why.

We are accustomed to viewing the pupils under distance fixation somewhat as shown in (A), and under reading fixation as shown in (B), Fig. 1. Superficially it seems safe to assume that if the distances between these two sets of pupils be accurately measured all will be well. That such an assumption is not warranted, however, may be seen by glancing at A and B, Fig. 2, wherein A represents distance and B reading fixation. In A the points F and F′, P and P′, L and L′ are equidistant hence the lenses L and L′ are correctly centered for the distance interpupillary measurement P and P′ so long as these two distances are equal. But in B, because of convergence, the foveal points F and F′ swing out, the

pupil centers P and P′ swing in, and in order to have the lines of direction pass through the centers L and L′ of the correcting lenses these points must be correspondingly closer to each other than are the centers of the converged pupils.

By holding a scale in the plane of these lenses and sighting with one eye from the reading point a fairly accurate reading measurement might be obtained but the operation is not as simple as it may seem and the calculation from the more easily obtained distance measurement is on the whole more trustworthy.

Having the distance interpupillary measurement the reading may be calculated by the following simple rule:

Rule 1.

For a reading range of 40 cm. or 16 inches:

Deduct 4 mm. from all distance measurements from 53 and 58 mm.

Deduct 5 mm. from all distance measurements between 59 and 75 mm.

Extremely few cases will be found to fall outside the above range but should it happen that we meet them we deduct 3 mm. from all distance measurement from 52 mm. down for reading and, again, deduct 6 mm. from all distance measurements from 76 mm. up.

Rule 2.

For a music or working range of 80 cm. or 32 inches:

Deduct 2 mm. from all distance measurements between 52 and 75 mm.

Extremely few cases will fall outside the above limits but it may be well to remember that 1 mm. is to be deducted from all distance measurements below 52 mm.

It has been assumed throughout that in actual practice no one will question seriously the advisability of obtaining the interpupillary distance by measuring from the center of the bridge of the nose to the center of the pupil of one eye and adding that to the corresponding distance from the same point on the nose to the center of the other pupil. Nor can we too strongly recommend that these two parts as well as the whole be recorded. Thus, for instance 30-30=60 mm.

Such a method of recording these measurements will facilitate greatly the selection of the proper sized frame or mounting and lens. To those who have not measured this way it may come as a surprise to learn that in remarkably few cases are the eyes symmetrically placed in relation to the center of the bridge of the nose, a fact however, which should be generally recognized and acted upon. In such a case the record might be 29-31=60 mm.

The instructions offered with either of the two more elaborate gauges referred to above recommend that the first number be that for the left eye. This we think unfortunate and worth changing for three reasons. First it is backwards because it seems self evident that in the above example one would naturally refer 29 to the right eye of the patient, and 31 to the left, as he looks at that patient; this is the order in which it

would be seen in the gauge. Secondly, all through our work we record right eye findings first. And thirdly, the technique of applying the gauge itself is improved by this method because it permits the slide, while the right eye is measured, to be pulled over against its spring and then allowed to slide back gently under its own tension which insures far greater steadiness of the gauge on the nose than is possible when the slide is pulled against its spring from before the right eye after the left has been measured first, an operation that almost invariably causes the instrument to move on the nose and, of course, vitiate the result.

With the simple, slide-rule gauge, Figs. 3, 4, 5, the directions for use call for measuring the patient's right eye first and left eye second and recording these two measurements in that order. One outstanding advantage of this simple rule is that each slide not only affords extreme accuracy in locating the center of the pupil and in reading the scale but remains fixed as long as it may be needed, that is, until the record is completed.

So there seems to be no reason, outside the ultra-conservatism of adhering to a custom because it is old regardless of its inherent inaccuracy, why a national or international standard should not be established along the lines here outlined. Not only will such a standard eliminate much confusion and many errors but it will permit comparison and tabulation of data, at present impossible.

Summary

1. The interpupillary distance should be the first measurement in the optometric examination, instead of being relegated to the end.

2. From there on through every test to the finished glasses this interpupillary distance should be strictly co-ordinated so that all false plus and minus prism values may be kept out.

3. By such procedure a wider tolerance is allowable because any inaccuracy likely to result is automatically compensated.

4. Of the many pairs of points representing the interpupillary distance that representing the approximate center of each pupil is advocated.

5. A simple, slide-rule gauge is offered with which to measure.

6. The distance measurement only is taken by means of the gauge while the reading is deduced from this by a very simple table.

7. The value of such a procedure being adopted as an international standard is suggested.

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THE LIMITATIONS OF DYNAMIC SKIAMETRY*

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In the thirty years that have elapsed since A. J. Cross presented his theory of dynamic skiametry, no investigator in the field seems to have doubted his major premise that, the accommodation and convergence being reflexly related, the accommodation will surrender to the lenses in the trial-frame only that degree of power that represents the excess over the amount normally associated with the convergence at the distance tested. It is true that Sheard[1] claimed that there is a physiological *lag* of the accommodation behind the convergence at near, and that part of the convergence innervation is caused, not by the accommodation, but by the fusion centers. Nevertheless, his interpretation agrees fundamentally with Cross' assumption and so do the interpretations of subsequent investigators, however sharply they may disagree on methods of procedure, explanations of the "lag," significance of tonicity tests for near and far, etc. Especially is this true of Tate's system[2], the latest and most comprehensive in the field, which seeks to determine the amount of convergence-accommodation in each individual case by correlating the dynamic findings with the results of tonicity tests for near and far.

The writer seriously questions this major premise, but before going into a discussion on the matter, he wishes to refer to an important technical point that seems, surprising enough, to have escaped the attention of investigators, judging at least by their published articles on the subject. This is the all-important point of binocular fixation. When dynamic skiametry is practiced in a dark room with both eyes of the patient fixed on the letters attached to the retinoscope, the eye under observation *is not focused on the fixation-target as indicated by the fact that it does not see the letters at all.* If the reader wishes to convince himself of this fact, let him sit in the dark room with one eye occluded and, holding a self-luminous dynamic retinoscope 13 inches away, let him flash the light into his own eye at such an angle that the image of the bulb in the skiascopic mirror will be visible if looked at directly. He will then be in the position of the patient. If he does this, he will find that his eye is so dazzled by the light that it will be impossible for him to make out a detail on the

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fixation-target. When dynamic skiametry is practiced under these conditions, it is the eye that is *not* under observation that is fixing the target, as shown by the fact that if the reader exposes the eye that was occluded, he will be able to read the letters for the first time. In other words, the conclusions of investigators who have experimented under these conditions and who base their interpretations on convergence considerations, are subject to suspicion because, instead of examining the eyes binocularly as they thought, they were unwittingly examining a dazzled eye with suppressed vision.

Now there are two ways of overcoming this difficulty but both of them involve other difficulties. One way is to move the fixation-target further away from the peep-hole of the mirror. The trouble with this plan is that, if the letters were moved far enough away to overcome the dazzling effect, the dynamic findings would be vitiated both by the aberration caused by obliquity of the incident rays and by the fact that the area of the retina refracted would be too far removed from the fovea. The other possibility is that of practicing dynamic skiametry in a well-lighted room and diminishing the intensity of light reflected from the skiascopic mirror. This plan, while more advisable, is open to the objection that it increases the difficulty of seeing the reflex clearly, especially when the pupil is small. It is difficult enough at times to secure definite findings even in a dark room with the usual intensity of light in the skiascopic mirror. To quote Sheard[3]: "Many skiametrists report difficulty in the matter of judging when a condition of neutral shadow is present and many have written, stating that they find a considerable range of lenses which afford neutral shadows." It would be proportionately more difficult and the findings would be still more independable if the illumination were cut down either in the examining room or the skiascopic mirror or both.

With the limitations of the dependability of dynamic findings clearly in mind, let us now turn to a consideration of Cross' basic assumption which underlies all current interpretations in dynamic skiametry. Young, emmetropic eyes, say, will accept a certain amount of plus power dynamically up to a certain point and no further as shown by the fact that when the power is increased beyond this point, there is a reversal of the skiascopic shadow. Why? Because, it is held, the amount of accommodation locked reflexly with the convergence at the distance tested comes into play, in spite of the offered assistance of convex lenses. It is time, however, that we began to regard this conception with suspicion. If it were valid, then our dynamic findings (so-called) would be relatively different when the convergence is eliminated. This, however, is not true. *If the eye that is not under observation is occluded, we will generally if not always find that it takes the same amount of lens power to cause neutralization of the shadow as when both eyes are exposed.*[4] Let the reader test this claim out on his next dozen cases. It is necessary, as stated before, to use reduced illumination when one eye is occluded.

It has been definitely established, especially by means of a beautiful experiment devised by Maddox[5], that an occluded eye will turn out 3 or 4 degrees when its mate is fixed on a near object—proving that under these

conditions convergence is abolished. The fact that under these conditions, the same findings obtain normally in dynamic skiametry, as when convergence is in use, would seem to indicate that we must look elsewhere than in the so-called accommodation-convergence reflex for an explanation of the problem. It seems to the writer that the logical explanation is an obvious one and that it would probably have been adopted long ago had it not been for the hypnotic influence of Cross' pioneering assumption. That explanation is the irrepressible functioning of the ciliary. The continual demands made upon this delicately responsive muscle seem to have produced what may indeed be termed a conditioned reflex to the exciting object in spite of the offered assistance of lenses. The familiar necessity of repeated doses of atropine in many cases before the ciliary is entirely relaxed testifies to the remarkable degree of its reflex activity. It would be surprising indeed if the mere juxtaposition of a lens between it and the object could succeed in inhibiting completely so habituated a response.

This explanation is preferable to the traditional one because it rests on a less dubious assumption, namely, the accommodation-convergence reflex.* It helps to explain why two individuals with the same condition of static and dynamic refraction, muscle balance for near and far, etc., may yet require different powers to bring the shadow to the point of reversal, since the degree of reflex play of the ciliary may differ in the two individuals. It is a simpler hypothesis, less dependent on rationalization in dubious cases and equally applicable in all cases, whether the convergence and accommodation are reflexly related (as when the removal of the hyperopic correction causes immediate squinting in of the eyes) or whether the convergence and accommodation are independent functions (as in external ophthalmoplegia).

It has been found by several observers[6], experimenting on young emmetropes with normal far-point and near-point tonicities that O. U. plus 1.50 D. sphere is required dynamically to bring the shadow to the point of reversal at 13 inches; and in Tate's system[2] this amount is used as a norm to represent the usual amount associated with convergence at 13 inches. According to the writer's explanation, the remaining 1.50 D. of accommodation that is exerted by the ciliary simply represents the normal amplitude of reflex play, assuming the findings to be correct, and has no significance necessarily as far as the convergence or lack of it is concerned. The plus 1.50 D. in the trial-frame represents the negative range of accommodation at 13 inches, as Sheard claims, plus the so-called *lag* of accommodation behind convergence.

In regard to the "lag," the writer suggests that it be dropped from the vocabulary of the refractionist and the term "dynamic aberration" substituted instead. After Southall's masterly exposition in the Journal of the Optical Society of America[4b], to use the word "lag" in reference to the "with" movement of the shadow observed in dynamic

*The fallacy of this theory is indicated in "The Accommodation-convergence Relationship—a Re-Formulation" which is soon to appear in this Journal.

skiametry with emmetropic eyes, is like continuing to use x in an equation after x has already been solved. Southall showed that, owing to the monochromatic aberration in the eye, the change in refraction that it must undergo when changing its fixation from far to near, in order to secure conjugacy with the fovea, does not correspond to the dioptral value of the nearer distance. The amount of the change can be calculated according to Gullstrand's equation as follows:

$$\Delta A = -\frac{(A + F)^2}{A + F + 1000n'/e}$$

where ΔA denotes the accommodation in dioptres necessary to bring the nearer object to a focus on the fovea; A the static refraction of the eye (for example, —2.00 D. in a case of myopia or +2.00 D. in hyperopia or 0 in emmetropia); F the refractive power of the eye (which is +58.64 D. according to Gullstrand's schematic eye); n′ is the index of refraction of the vitreous humor (1.336 in Gullstrand's schematic eye); and e represents the distance between B, where the optic axis meets the retina, and N, where rays from the nearer object cross the optic axis before the eye is accommodated for it. e is reckoned positive or negative according as N is beyond B or between B and the crystalline lens respectively.

Suppose we calculate for ΔA when the object to be fixed is 13 inches or 33.3 cm. away, as it is in dynamic skiametry, and assuming we are dealing with a normal, young emmetrope. We have all the data to substitute for the symbols in the above equation (thanks to the constants in Gullstrand's schematic eye) except e, which represents the distance the incident rays cross the optic axis behind the retina when they are coming from a point-source 33.3 cm. away from the cornea and the eye is gazing at infinity. We can calculate for e by applying[7a] the formula:

$\frac{n'}{u'} = \frac{n}{u} + \frac{n'-n}{r}$ to each of the six refractive surfaces[7b] of the eye in turn, taking into account the distances between the surfaces, where n represents the index of refraction of the first medium traversed; n′, of the next medium traversed; u, the object-distance (it must be remembered that the image formed by refraction at one surface, serves as the object in reference to the next surface); u′, the image-distance; and r, the radius of curvature of the particular surface. If we thus calculate the path of a bundle of paraxial rays issuing from a point-source 33.3 cm. away from the anterior surface of the cornea, we will find that e is +1.6 mm.

Substituting +1.6 for e in Gullstrand's equation and solving for ΔA, we find that the latter is —3.84 D. In other words, in order to secure conjugacy on the fovea, it is necessary for the eye to accommodate 3.84 D. if the object is 33.3 cm. away. As it accommodates only 3.00 D. instead, the observer in dynamic skiametry will notice a "with" movement of the shadow which is arrested by about +.84 D. sphere, the discrepancy

between the lens power actually found and the deficiency calculated being due, among other things, to the fact that the values used are those of Gullstrand's schematic eye. The .84 D., however, is near enough to the .75 D. of so-called "lag" usually observed to warrant the conclusion that the monochromatic aberration of the eye is the chief, if not the only factor, that explains the "with" movement of the shadow that obtains under the stipulated conditions.

Why, it might be asked, does not the emmetrope see small reading print slightly blurred, owing to the refractive deficiency caused by the aberration? We know that a deficiency in static refraction of .75 D. will cause the 20-20 line of the Snellen test chart to be definitely blurred (assuming, of course, total absence of accommodation), although the 20-30 line will be read with difficulty. The answer seems to be twofold. In the first place, the smallest reading print in use is appreciably larger than the size of letters at 33.3 cm. that would correspond to the 20-20 distance line. The Snellen letters subtend an angle of 5′ at the distances specified. In order to subtend this angle at the nodal point at 33.3 cm., the size of the reading letters would have to be 333.3 mm. $\times$ tan 0 5′ or 0.485 mm. The small letters in the addresses of the telephone directories are more than twice as large as this. Even the smallest type normally available, namely, that used in the printing of pocket Bibles is substantially larger, at least vertically. Therefore, in testing the near vision, we do not use the same standard of measurement as for distance; in fact, the Jaeger No. 1 subtends about 10′ instead of 5′ at 33.3 cm. Besides, the illumination for near is relatively brighter than for distance of and intensity of illumination is a vital factor in visual acuity. Then again the individual has the ability, denied him for distant objects, to hold near objects closer to his eyes so that they subtend a larger angle.

In the second place, the blurring effect caused by the aberration is partially overcome by a contraction of the pupil, especially as the letters approach 0.48 mm. in size. This point was investigated as follows: Normal, young, emmetropic eyes (or eyes made emmetropic with glasses) were examined with a retinoscope while they fixed large letters attached to the instrument. Noting the size of the pupil and without moving the retinoscope, we quickly substituted for these letters a small card of diamond type. Generally, as soon as the eyes focused the small print, a definite decrease in the size of the pupil was observed. Changes in pupillary size are particularly well revealed by the retinoscope owing to the sharply defined edges of the luminous area. No perceptible increase in accommodation was noted in any case, although it is possible that less than .25 D. of accommodation that might have been associated with the pupillary contraction, could not be detected. At any rate, contraction of the pupil, by decreasing the diameter of the circles of diffusion, produces substantially the same effect as a slight increase in accommodation.

It is probable that normally the eye does not have to over-accommodate or over-contract its pupil to overcome the aberration, owing to the

relatively large size of the fixation-objects ordinarily in use; just as an individual with incipient presbyopia is often not aware of his deficiency provided he confines his gaze to relatively large objects. It is only when objects or letters approaching 0.48 mm. in size are fixed—especially if they are fixed for a long period of time—that the excess tension of the sphincter iridis in each eye (if not the excess accommodation) may produce asthenopia. This is probably the most important reason why the recent change to larger newspaper print is so desirable, rather than the reasons usually given. It is this dynamic aberration that may explain why workers in fine details like diamond-cutters, engravers, etc., often require special working lenses in spite of the fact that they can read ordinary print for hours with no discomfort. Finally it explains why young emmetropes, especially of the neurasthenic type, sometimes experience such surprising relief through the use of weak convex lenses for sewing and reading only, even though their accommodative reserves are ample and their muscle balance is normal.

In conclusion: the method of dynamic skiametry so-called (which requires the use of letters attached to the retinoscope, with fixation—and observation by the refractionist—at 1 meter or, preferably, less), while questionable as far as investigating accommodation-convergence relationships is concerned, is nevertheless of great value as an objective determination of the monocular refractive condition of each eye when it is fixing a near object. This statement is made in spite of the technical difficulties referred to previously. When taken in conjunction with the correction discovered by static skiametry, in which the gaze of the patient is directed off at the distance, the dynamic findings are particularly valuable in cases of premature presbyopia, spasm of accommodation, weakness or paresis of accommodation, and lenticular astigmatism—especially in those cases where the axis of the total astigmatism changes when the gaze is directed from a far to a near point.

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CHROMAGRAPHOLOGY

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Chapter 5.

Chroma Interpretation—Normal and Abnormal

In considering now visual interpretations of chroma we are immediately faced with the sometimes debatable issues of our subject. Debatable because, due to instrumentation which probably did not conform to basic principles that would have eliminated much of the question producing debate. As in any other scientific work, the greater the number of variables involved the less certain become the results obtained, and, it is often a question in the mind of the reader as well as the writer whether such debate and its resultant effect upon the authenticity of anything accomplished, is not very weakening to the practicability of the entire structure. Of course it is not to be discarded or disregarded, that certain standards must necessarily be set in order that we progress in the adaptability of any technique, and for that reason this chapter will be devoted to the study of all the problems raised with an endeavor to give a complete consideration. It will be the aim of this chapter to make a contribution to our knowledge of the normal and abnormal chroma interpretations. In so doing it will be necessary to present data obtained by the writer in a series of experiments, and at the same time to correlate this data with the theoretic principles advanced from time to time by other investigators. An unequivocal and undisputed statement of the facts of the case is of course a prerequisite to satisfactory progress in the domain of theory.

With uncertainty prevailing there can be no definite and certainly no unanimous envisagement of the subject, and in late years there has been a distinct tendency to harmonize the findings of various experimenters, who have attacked the problem.

This brings to mind a fact which most of the readers are aware of, and that is, that at just about the time we in ocular work have thought that we had a basis of understanding in field work, some investigator has produced a statement or paper entering a disturbing note and thereby delaying progress in the field. In other words it seems that unless investi-

gations have a real meaning involving great changes in our techniques for charting visual interpretations, it may be that they will only suffice to cloud the issue with little to be gained. Of course we cannot let that statement deny such investigators of the credit they deserve for the valuable contributions they have offered in the solution of the problem. In fact it is by virtue of these thoughts that other investigators are encouraged to grapple with the subject anew, and we see every day a new thought developed which may revolutionize our entire procedure.

After Mariotte's discovery of the blind spot a great many examinations of the peripheral retina were instigated, and Thomas Young is probably the first to explore the retina as to its areas of sensitivity. Troxler attacked the problem independently a few years later and discovered characteristic properties differing in the periphery and the central regions. He brought out for instance the fact that images falling upon the periphery and mid-periphery were inclined to fade out rapidly and disappear while those in the central regions were more fixed. In his opinion the degree of persistence was conditioned by two factors, the colour of the object, and the angular distance of the object from the fixation point. As to the first of these he brought out the fact that they seemed to reappear several times before ultimately disappearing entirely. As the reader can very easily see, this might be occasioned by the fact that the fixation may have moved so that the objects in rapid succession would come into and go out of the field of recognition. A little later on Purkinje confirmed some of the observations of Troxler and attempted to relate them with certain other subjective phenomena, and in so doing he discovered that coloured objects appear gray when their images fall upon peripheral parts of the retina, but he said that it was difficult to determine whether the clearer vision of the central part was due to the presence, at the fovea, of a greater light intensity or to a pecular structure at that point. He was also the first to observe that sensitivity to white had a wider retinal extension than sensitivity to colour, and that the different colours have retinal zones of different extension.

Following Purkinje was Szokalsky who conceived the retina to be made up of three concentric zones, and although he did not elaborate in detail he assumed the outermost zone to be sensitive only to black and white, the intermediate zone only to blue and yellow, black and white, while the innermost zone was capable of sensing these four and red besides. A little later on, Huck endeavored to determine the minimal extent of a retinal image capable of producing a sensation of colour. His essential contribution was his discovery that the colour zones are not fixed.

By far the most fruitful investigator of this period was Hermann Aubert who, unable to find satisfactory detail in literature referring to the phenomena of colour, determined to undertake a thorough investigation of the phenomena of indirect vision and for that purpose he employed an apparatus described in Chapter 2 as having been revised by Forester and known as a perimeter. The problems according to a report of the Carnegie

Institute which he set himself to solving were as follows:

"(1) To what extent does the character of the background influence the degree of eccentricity at which a colour stimulus can be recognized?

(2) What is the relation between decrease of colour sensitivity and decrease of area of stimulus?

(3) Does colour sensitivity decrease at a uniform rate along the various retinal meridians?

(4) Through what transitions of tone does each colour pass during the movement of its image from the fovea to the periphery of the retina?

(5) What is the relative extension of the retinal areas within which stimuli appear in their true colours?

(6) In what degree are the phenomena of indirect vision analogous with those of direct vision?

Summarizing his results:

(1) The brightness of the background has a most pronounced influence upon the extension of the colour sensitivity and of the brightness sensitvity. He found that red against a white ground became colourless at 16 degrees, red upon black at 30 degrees, blue upon white became colourless at 13 degrees, and blue upon black at 37 degrees.

Further, he concluded that beyond the limits of the colour zones all colours appeared white when the ground was black and black, when the ground was white.

(2) The extension of the colour zone increases with the increase of the area of stimulus, but not in the same proportion; the extension of the colour zone does not increase as much as might be supposed from the increase of the area of stimulus.

(3) The colour sensitivity increases at different rates upon different retinal meridians, diminishing much more slowly and extending much farther out on the nasal side of the retina.

(4) The transitions of the colour tones were as follows: Red passes through reddish yellow and yellowish gray to gray; green becomes yellowish, while yellow and blue undergo no change of tone, but decrease in saturation, finally appearing gray.

(5) The relative extension of the different colour zones cannot be determined with any degree of accuracy. (This of course was because of his changing the luminosity of the stimulus and by considering the varying degrees of brightness and probably many other variable factors, which after over 70 years is still being carried on by many praiseworthy investigators, to what end it is hard to determine.)

(6) There is a close analogy betwen the function of the central and peripheral parts of the retina. (This determination again being brought about by the fact that he was changing constantly the size of the objects hence the visual angle subtended. Again, to what end is not known.)"

Then Herman von Helmholtz assumed that the red sensing substance

must be lacking at the periphery, hence a red blindness of this eccentric region.

Investigator followed investigator until we come to Landolt who by virtue of his work determined that no part of the normal retina is wholly insensitive to colour of any tone, that colour-blind was an inappropriate definition and could not properly be applied even to the extreme periphery. (He evidently neglected the difference in structure between the various parts of the normal retina.) This therefore became a definite rejection of the explanation offered by Helmholtz. Then in about 1873 R. Schirmer in his studies of the abnormalities of colour vision found that in progressive atrophy of the optic nerve the colour zones gradually decrease in area, until in the final stages of the disease the pathological macula became functionally equivalent to the normal periphery. His work brought out that the sensitivity to green is the first to go, then that to red, to yellow, and finally that to blue. Schön at about the same time found green to have the smallest colour zone, blue the largest and red occupying an intermediate position. According to the different retinal sensitivities to the different colours, green requiring the greatest luminosity, red less, and blue least of all. Passing by some of the other authorities who made investigations at that time we come to F. C. Donders, who, it seems, was the first to suggest to Landolt that all colours could be perceived upon the outermost regions of the retina if only the stimulation be sufficiently intensive. He qualified that by saying that the color sensitivity of the periphery is so weak as to approximate in some degree the condition of the diachromatic or even of the monochromatic retina. In other words the periphery possesses a colour sense which is simply less highly developed than that of the fovea.

(If we believe that the cones are more highly developed rods it would mean exactly the same thing in the opinion of the writer.)

Ole Bull in 1893 and 1895 made some important contributions to the literature of the topic, concluding that the colour sensitivity of the periphery differs in many respects from that of the foveal area, and that the periphery does not function in a manner which is comparable to the center in twilight illumination (light adaptation and dark adaptation.) In 1886 Dobrowolsky attempted to determine the relative sensitivity of the various parts of the retina and concluded by determining that the colour sensitivity is keenest at the fovea and that it decreased gradually towards the periphery and that the decreases differ for different colours and for different parts of the retina. He also made a very valuable contribution in that he determined that practice tends to extend the limits of the colour zones. (Myologic technique has made use of this in correcting cases of chroma amblyopia.) Following him in 1886 came Albini who made experiments, designed to determine the effect of the error of refraction existing in the periphery upon the peripheral sensitivity to colour. Among his conclusions we find that the extension of the colour zone is invariably increased by the convex lense and that the increase of a given colour zone is directly proportional to the refractive power of the lense employed, with this im-

portant exception; it has the greatest influence in the case of blue, less for red, and least for green. Including with this statement of an increasing error of refraction in the periphery as we approach the orra seratta, he also mentions the inferior training of the extra-foveal regions as a subsidiary principle of explanation.

V. Vasevi conducted a series of adaptation experiments in which he determined that the extension of the colour zone is increased by dark adaptation. He then performed another series of experiments to determine the influence of chromatic adaptation, subjecting the retina to an exposition of each colour for a time and then presenting each of his stimuli at their minimal brightness increasing in degree until the colour was recognized.

In an issue of the American Journal of Psychology in 1895 G. W. A. Luckey made a comparative study of the retinal extension of colour sensitivity in individuals who differed in age, sex, and training in colour discrimination, finding that children have narrower zones than adults; that there is no variation correlate with difference of sex, and little or no increase of zone extension as a result of previous training in colour discrimination.

The foregoing investigations extending over a period of approximately 70 years has been, as will be seen by the reader, a very disturbing factor in the progress of actual adaptation of colour field studies to the average practitioner. It is true, however, on the contrary, that from the standpoint of laboratory experimental work it has been of extremely great interest and probably of value. Comparable with these experiments which were conducted largely with the original form of perimeter, we have the experiments which are today being conducted by Dr. C. E. Ferree and Dr. Gertrude Rand, using the Ferree-Rand Perimeter. As has been stated before, despite the fact that the Ferree-Rand Perimeter is a most wonderful monument to the accurate scientific work of its inventor, the principles have not been changed from the original perimeter although the technique has been improved and it is the modest question of the writer concerning the value of going over the same investigation again.

In the American Journal of Ophthalmology Vol. 12, No. 4, April 1929, Dr. Ferree and Dr. Rand in collaboration with Dr. Monroe presented an article under the heading "Studies in Perimetry" based upon careful and accurate laboratory work which brought out four reasons why there is a greater range of variations in the colour field than of the so-called form field (Visual field for white stimulus), as follows:

"(1) The colour fields demand a much more careful method of working than the form field. The success of the examination is also much more dependent on the intelligence of the observer, the instructions received, and the amount of preliminary training and practice that is given. Practice in making the judgment of colour is without question a very important factor. The results are more dependent too on the skill and experience of the operator and his intelligent understanding of the characteristics of the chromatic response of the peripheral retina and the factors by which

it is influenced. The difference between the methodologic problem set by the two types of determination can not be too strongly emphasized at this point.

(2) The colour fields are much more sensitive to conditions which may influence the results, both pathologic and nonpathologic, than the form fields. This superior sensitivity gives them much greater possibilities as a means of diagnosis and investigation, but it also makes them more difficult to use. A sensitive instrument, for example, because of its sensitivity, is apt to be erratic. The satisfactory use of such an instrument in the hands of a novice is impossible. When used by a skilled operator, however, and properly protected from the influence of extraneous factors, it has an advantage and degree of usefulness in direct proportion to its sensitivity.

(3) The colour fields are more subject to variation due to individual differences than the form field. All of the factors which differ from individual to individual have in general more effect on the colour fields than on the form field because of the difference in sensitivity just noted. In addition there are greater individual differences in chromatic than achromatic sensitivity and greater deviations in its irregularity of distribution over the retina. Central colour sensitivity has been found to sustain such an important relation to breadth of field in the nonpathologic eye that it may well be that the diagnostic scale should be formed and used in conjunction with tests for central sensitivity. That is, by means of the two scales determined for the same observers, one showing the normal range and distribution of breadth of field, we should have not only a two-fold check on diagnosis, but a scale of relations, disturbances of which might have an important diagnostic significance. The relative amounts of effect on central and peripheral sensitivity may, for example, prove to be an important aid in differentiating between health and disease and between the various pathologic conditions.

(4) The use of a small stimulus increases the sensitivity of the perimetric method more for the colour than for the form stimulus. Proportionally greater facility is thus given to colour perimetry for picking up differences between observers. This furnishes an additional reason for expecting a greater scatter of results for a one degree colour than for a one degree form stimulus. It may yet be found to be of advantage in the case of colour to use a larger stimulus (e. g., two degrees) for diagnostic work based on size of field and to employ small stimuli and other devices for increasing sensitivity only for the detection of scotomata, regional cuts, and so on, and for checking up the advance and recession of a pathological condition, in which cases the increased sensitivity entails no compensating handicaps.

It is obvious that much has yet to be done for colour perimetry before it can be given maximum serviceability in diagnosis so far as extent of field is concerned. Results are presented at this time, in part to serve as a tentative scale until a more thorough study can be made of the factors which differ from individual to individual, and in part by way of

admonition to show what may be expected as to the normal range of variation of field when taken under conditions which approximate those found in the clinic, even when all external conditions known to influence the results are carefully controlled.

In this connection ,it scarcely need be pointed out that the value of a scale for diagnosis does not depend upon the normal range of variation alone, but upon this range in relation to the pathologic range. A large normal range is of consequence only in so far as it increases the liability to overlapping with the pathologic range. Data on this point will be presented in a later paper."

Following this in August, 1929, Doctors Ferree and Rand collaborating with Doctor Monroe had another article appearing in the American Journal of Ophthalmology in which they discussed the effect of errors of refraction, age and sex in relation to the size of the form field (this would be better named the field of white stimulus). So that one immediately sees that there is a certain lack of belief even in this age old procedure. Two hundred individuals were studied on the Ferree-Rand perimeter, with strictest observance of all the precautions so important in this type of work. The illumination was kept constant and the fields were determined with a white stimulus on a black background. All cases of pathological condition, no matter how slight, were excluded. Ample rest periods were given, and it is said that care was exercised that no field should be taken while the observer was suffering from general fatigue or was otherwise unfit for accurate work (it must be remembered that the problem of controlling fatigue and mental stagnation is indeed a hard one even for such able authorities as Doctors Ferree and Rand). These two hundred subjects were obtained so that there might be a good mixture as to errors of refraction, of course including hyperopes, myopes, astigamats and emmetropes; age ranging to sixty years of age; and of both sexs. Observation of the tables produced after these studies show the following which is so reported by the observers:

"Inspection of these tables indicates that there is some tendency for size of field and age to vary concomitantly, the larger fields occurring at the younger ages. The coefficient correlation between age and size of field computed by the product-moment formula of Pearson for linear relationship is -0.31×0.04.[3] The table shows, however, a slight nonlinearity of relationship due to a pronounced shift towards smaller fields near forty years of age. That is, above forty years the mode (the value on the scale that occurs most frequently) falls in the group 66 to 69.9 degres (average 68); while below forty years the mode falls in the group 62 to 65.9 degrees (average 64). Between thirty and forty years a transitional stage is seen where there is more nearly an equal number of cases in the groups whose averages are respectively 68 and 64.

This shift of the mode downward at forty years of age and the transitional stage between thirty and forty years will be shown in a later paper to occur also for the colour fields. The average size of field for the age group ten to nineteen is 67.0 degrees; for the age group twenty to twenty-nine is 67.5 degrees; for the age group thirty to thirty-nine is 66.8 degrees; for the age group forty to forty-nine is 64.8 degrees; and for the age group fifty to fifty-nine is 64.7 degrees.

It may further be noted that no very large fields (more than seventy degrees) occur above the age of forty and no very small fields (less than sixty-two degrees) below the age of twenty years. These characteristics are also in general found for

the colour as well as for the form fields.

The narrower fields of the presbyopic group are probably largely a phenomenon of age rather than an effect of presbyopia. That is, presbyopia, the loss of sensitivity, and the general failure of all the powers of the eye are in the main coordinate rather than interdependent phenomena of age. However, in the more advanced cases of presbyopia the defective focusing for close vision, due to loss of accommodation, doubtless exerts a direct influence on the results of perimetry when the stimulus is viewed at a distance of 33 cm. Other age phenomena, extraneous to the functional changes in the sensory mechanism, which may exert an influence on the perimetric results are opacities of the cornea, lens, and vitreous; changes in size of pupil; and so on.

The results have further been studied to show what relation, if any, exists between sex and the size of the form field. The 200 cases studied included 84 males and 116 females. The median value of size of field rated in terms of average breadth is 66.4 degrees. For the males the average breadth of field for thirty-six cases, or forty-three per cent of the number tested, falls above and for forty-eight cases, or fifty-seven per cent of the number tested, below this value. For the females, the average breadth of field for sixty-four cases, or 55 per cent of the number tested, falls above and for fifty-two cases, or forty-five per cent of the number tested, below this value. The average breadth of field for the males is 66.04 degrees; for the females 66.75 degrees."

In order to simplify the reader's understanding of the foregoing let us associate very briefly some of the highlights or major points of this work by saying that after these authorities had supposedly separated the pathologic cases from the nonpathologic, which must be very hard to do if we keep in mind the fact that pathology does not develop in a few hours but is the result and development of long standing and continued failure in the human body, they eliminated the pathologic and confined their observations to the nonpathologic. These nonpathologic cases were then sampled to include as many as possible of the outstanding variables, such as errors of refraction, age, and sex. Their determinations evidences that errors in refraction were an important factor influencing the variability of result. Age became an important factor only after 40 years, sex having such a small amount of effect as to be negligible.

It is not the purpose of the writer to deprecate the value of this eminent piece of work in the slightest degree, but it is desired that we use these observations, not to reduce the usage of field charting, because of a thought that it is unstable, but rather to clarify our understanding of the results obtained in each individual's work. Therefore we may expect to find in the case of errors in refraction, slightly smaller fields associated with myopia and slightly larger associated with hyperopia. It may be that at some future time sufficient clinical evidence will allow the establishment of a differential which when multiplied or divided by the errors in refraction will come somewhere near giving us an idea of how much difference to expect. That, however, is quite in the future. As regards age, it is certainly most natural to expect slightly larger fields in youth and slightly smaller fields in advanced age. For instance, it may be assumed that in presbyopia the acknowledged senile developments, including loss of sensitivity and general failure of all the powers of the eye, are a part of the phenomena of age. Of course, there is always the exception to the rule which might be expressed by saying that in quite advanced cases defective focusing will have a direct bearing on such field charting as is

taken at or within one-third of a meter. Concerning sex, although the work of Doctors Ferree, Rand and Monroe implies that it is a negligible factor, the writer wishes to register disagreement by saying in advance of some material to appear in a later chapter which will substantiate, that sex is most certainly a factor, and we may expect to find smaller fields more often in the female than in the male, particularly in colour field work.

The writer does not wish for those following this work, Chromagraphology, to be confused by the figures of field limits given in the discussion above, because they in turn are controlled by the size of the stimulus, the illumination and many other variable factors, and further, because they are regarding the field of white, while in this work we are particularly confining our attention to colour. It has been presented, however, in order that both the reader and the writer may have taken note of this important work. If one attempts to judge as to whether Dr. Ferree and his associates are at this time amenable to colour field studies, the task would be quite hard, for in some of their statements they agree that it is a much more delicate and sensitive test and that therefore it may be more helpful in so-called borderline cases and at the same time they point out that there are so many variables involved that it might be interpreted as expressing a doubt as to the practicability. In fact at the time of the writer's last visit in the Wilmer Ophthalmologic Institute the colour work was confined very largely to the blind spot. Regardless of this it still remains that we are concerned in this work with colour and the writer has ample evidence of its value, together with the fact that it is being successfully used.

There are so many conditionals, which fact is made apparent by the long period of study, that the writer wishes to present her a recapitulation as described by John Wallace Baird of the Carnegie Institution in which he said:

"(1) In moving across the retina, the images of coloured objects of moderate size and luminosity pass through certain regular series of transitional tones. On the basis of these changes of colour tone we may conceive the retinal surface to contain three concentric zones—an extreme peripheral zone upon which all stimuli of moderate area and luminosity appear colourless; an intermediate zone where they appear yellow or blue (two exceptions will be mentioned later), and a central zone where red and green appear.

(2) The position and extent of the colour zones are not fixed, but variable. Their area in any given case depends upon the momentary condition of retinal adaptation, and upon the brightness and saturation of the stimuli employed. (Other investigators have established the fact that zonal extension is also a function of the character of the background, the condition of optic refraction, and the magnitude of visual angle of stimulus.) Hence the absolute and relative extension of the zones may be varied at will. It is possible, for example, to choose a violet stimulus which can be recognized as violet at the extreme periphery, and it is equally possible to choose another stimulus of the same tone of violet which can not be recognized as violet at the center of the fovea.

(3) Of all possible colours, there are four and only four which undergo no change of tone in indirect vision. These are a purplish-red (non-spectral), a yellow (about 570 milli microns), a bluish-green (about 490 milli microns), and a blue (about 460 milli microns).

(These numbers represent the wave-lengths at the center of the spectral bands transmitted by our stimuli.)

(4) When stimuli representing these four stable colours are equated in white-value and in colour-value, the retinal zone upon which the red is recognized coincides with that of the green, and the zone of the yellow coincides with that of the blue. The extension of the yellow-blue zone is considerably wider than that of the red-green zone.

(5) When a colour stimulus is applied to an eccentric region of the retina, the colour rapidly fades out of the image. The fading process follows a gradual course, and passes through a regular series of transitions. Here, too, the order of change is through yellow (for stimuli of long-waved tones) or blue (for stimuli of short-waved tones) to gray. If the stimulation be sufficiently intensive or sufficiently long-continued, the complementary blue or yellow appears as a final stage. All of these changes occur in the presence of the stimulus.

(6) The stimulation of the peripheral retina is attended by characteristic after-effects. These may be described as being rudimentary after-images. They are analogous with after-images in that (a) they possess a complementary character; but they differ from after-images in that (b) they are (or at least were under our experimental conditions) wholly latent and unconscious (or sub-liminal). Their presence was felt, however, in the changed quality of the sensation aroused by subsequent stimulation. (c) A prominent characteristic of these after-effects is their persistent duration. They can be got rid of only by resting the eye for a considerable period after each stimulation. (d) As to their spatial attributes, our experiments justify only the general statement that they seem to extend over a wider retinal area than did the images which occasioned them.

(7) There seems to be no doubt that Hellpach's zone of complementariness is an artifact, and that its discovery is wholly due to the experimenter's failure to avoid retinal fatigue in his explorations. The peripheral retina is readily fatigued, and the fatigue-effects here persist with extraordinary tenacity. This characteristic is also present, though in lesser degree, at every other part of the retinal surface. When a colour-stimulus which has already fatigued any region of the retina is reapplied to the same or an adjacent region before the retina has fully recovered from its previous stimulation, the resultant sensation tends to appear either colourless or in a tone which is complementary to that of the primary sensation aroused by the same stimulus at that point. (It is, of course, incorrect to say that these fatigue-products are complementary to the "objective Farbe" of the stimulus as seen in direct vision. Such a characterization is true only in case the stimuli employed represent stable tones. In every case the tone of the "fatigue sensation" aroused at a given region is complementary to the primary or "non-fatigue" sensation aroused at that region. Thus a purple stimulus is attended by green after-effects only at those regions where the purple of the stimulus is recognized. Upon those regions where this stimulus appears yellow, subsequent stimuli seem to be tinged with blue. This is in accord with Hellpach's finding that red, orange, and yellow all appear bluish upon the "gegenfarbige Zone." Strangely enough, he reports that green appeared yellow-reddish and purple appeared yellow-greenish upon this outermost zone. This irregularity can be explained only on the assumption that Hellpach's green and purple stimuli were more intensive than the others.)

This phenomenon has been abundantly demonstrated in this and in numerous other investigations; it is unquestionably the same phenomenon which Hellpach reports. Hellpach is in error, however, in supposing that this complementary function is normal characteristic of the non-fatigued retina; he is no less mistaken in supposing that it is peculiar to the periphery alone. The same phenomenon can be made to appear at every part of the retinal surface. It is invariably absent from periphery and center alike, when care is taken to delay the application of the stimulus until the retina has fully recovered from the effects of previous stimulation.

(8) Our results are in accord with the Hering and Franklin theories of colour

vision. They can not be reconciled with any other theory with which the writer is acquainted."

The evidence registered by the subject matter of this entire chapter is that we have to deal with a very complex and varying technique and yet the considerations involved throughout have failed to include what seems to the writer to be the most important and the finally controlling factor. In other words, wave-lengths end at the retina and by virtue of that fact our test is a psychologic something measured in a subjective manner. In other words, colour field results indicate not only physical and physiological changes, but psychological conditions which have a great bearing on the two first named.

Although we have evidently been striving to set down, so-called normals, and have failed, there is still no reason to doubt that with a careful study of the results obtained, including a consideration of the variables involved, indicative information gained will have a very important bearing in our symptomatology. Therefore in Chapter 6 our attention will be confined to the diagnostic value of alterations and an attempt will be made to strengthen our convictions by a discussion as to the possible and probable theoretical reasons for such alterations.

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THE CHEIROSCOPE

Dr. E. E. Maddox in working on the non-operative treatment of squint has devised an instrument called the Cheiroscope. The principle of this new squint treatment device is to co-ordinate the movement of the eye and the hand, and with this in mind the device uses both the hand and the eye in its training periods, making the patient perform such acts as drawing, following moving objects, etc.

The Lancet, in a special article on this subject comments as follows:

"The main object in the treatment of commencing squint in young children is, of course, to prevent amblyopia in the squinting eye. In the great majority of children Nature provides a safeguard against squint by the development of the impulse to fuse the slightly different images on the two retinæ so as to produce binocular perception. In certain individuals, however, this power of fusion is weak or develops late, and it may even be altogether absent. These are the children who are likely to develop a squint if other factors in the causation of that condition, such as hypermetropia, are present; if the squint is confined to one eye, and is constant, the tendency is for the child to suppress the image formed by the squinting eye. In young children the eye soon becomes amblyopic in the absence of treatment; when the onset of squint is deferred till the child is older the degree of amblyopia is less pronounced, but the longer it has persisted the more difficult it is to overcome. C. A. Worth found that of 193 cases which were brought to him when the duration of the squint was only one-eighth of the life of the patient he was successful in preventing any degree of amblyopia in 165. In cases where the duration had been more than

one-eighth but less than one-half of the patient's life, the corresponding cases of complete prevention were 73 out of 188, and in the cases where the patient had squinted for more than half his life the complete cures were only 14 out of 604. Besides occlusion of the non-squinting eye the non-operative treatment of squint consists in the training of binocular vision and of the fusion sense. Hitherto this has generally been attempted by means of stereoscopic views and of the amblyoscope, by means of which the child is encouraged to combine the two images presented separately to the two eyes. In practice, however, it is difficult to keep the child's attention on the pictures, however childish these may be. It is to Dr. E. E. Maddox, the inventor of the "Maddox rod," that we owe a new principle in this sort of treatment. By means of his "cheiroscope" the child's hands, as well as his fellow eye, are made to assist in the development of binocular vision. He can trace the outline of a picture visible to one eye only with a pencil visible to the other eye alone, or he can play a game of catching a butterfly visible to one eye alone with a net held in the hand of the other side and directed by the other eye. Such devices in the hands of properly trained guides are without doubt a great advance on previous methods. In certain cases they may even effect a complete cure of incipient squint and forestall the necessity of an operation."

C. C. K.

TRANSILLUMINATION

While the writer is fully aware that many authorities no longer place emphasis on the findings of transillumination, preferring the use of the X-ray where possible, he fully realizing the weaknesses of transillumination, does nevertheless choose, to recommend this form of diagnostic technic to all optometrists.

The reasons for so doing are as follows: Focal infections in the maxillary antrum or nasal accessory sinuses are at times the cause of reflex ocular disorders which naturally will not be corrected until the focus of infection itself is eliminated. Transillumination, while failing in some cases it is true, does as a rule give the examiner a sufficient shadow-like clue as to the cause of the disorder to enable him to advise and insist on the further use of the X-ray, for additional diagnosis.

Transillumination is also used in connection with the eye although a different type of instrument is here to be preferred. The writer has used the transilluminator of Wurdemann for ocular diagnostic work for some years with splendid success. The instrument is smaller and different in shape than most transilluminators of its kind and is only used for searching the anterior portion of the globe of the eye for tumors or foreign bodies as well as to examine the size of opacaties of the lens although Fox claims that at times even a detachment of the retina can be faintly seen.

The technic of examining the lens for opacaties the writer has found of value in cases where the central portion of the crystalline lens is effected. Having the patient look up, the transilluminator is placed firmly against

the lower lid, the rays so directed that they pass through the sclera below the cornea. The interior of the eye is in this way fully illuminated and the lenticular opacaties will be found to stand forth as either grayish or black patches in an otherwise transparent lens, enabling the examiner to more easily note their size and shape, than by means of either focal illumination or ophthalmoscopy.

C. C. K.

CAUSES OF BLINDNESS IN OLD AGE

Dr. J. Jameson Evans, M.D., C.M., F.R.C.S., in a recent issue of the British Medical Journal analyses the causes of blindness in some seven hundred elderly patients certified by him for aid under the Blind Persons Act of England. His figures are as follows:

	No. of Cases.
Cataract	205
Atrophy of optic nerve	78
Glaucoma	75
Iridocyclitis and keratitis	54
Corneal opacities	45
Retinitis pigmentosa	43
Choroido-retinal atrophy	42
Congenital defects	33
Detachment of retina	27
Myopia	20
Phthisis bulbi	17
Choroido-retinal vascular disease	14
Diabetes	12
Ophthalmia neonatorum	9
Sympathetic ophthalmia	9
Trachoma	9
Buphthalmos	5
Injury (double)—two gunshot and one explosion	3

C. C. K.

THE SHEPARD SUBJECTIVE TECHNIC

A new subjective technic has been introduced to the profession by Dr. Carl F. Shepard, optometrist of Chicago. This technic may prove of value in examining small children or illiterates and because of its simplicity and its apparent ease of operation should prove an aid to all optometrists.

The technic is applied as follows: A skiametric examination is first made, both static and dynamic and with the approximate correction in place, one eye is entirely occluded and a pinhole disc is placed before the other eye. Next a white card is placed before the eye in front of which the pinhole disc has been placed and a small dot is made upon this card. The size of the dot is gradually increased until it is discernible to the patient. The dot, it is suggested, should not be made in the center of the card but rather to one side or in one corner so that the patient must search

carefully for it. As soon as it has been found, the pinhole disc is then removed and a fogging plus sphere is interposed, thus making the dot again invisible. The sphere is then reduced by quarter diopted steps until the dot again is perceived. At this point several light parallel pencil lines are drawn along the face of the card, each line approximately the same width as the dot. The card is then rotated before the patient's eye. Should there be an over correction in any meridian, the lines will disappear, indicating astigmatism and changes must be made in the correction to allow for this. These changes can be made with minus cylinders, thus getting the lines to appear equally distinct in all meridias. As soon as this is accomplished, similar tests are made on the other eye.

C. C. K.

BOOK NOTICES

OPTOMETRIC JURISPRUDENCE. Edited by Lawrence P. Folsom. Published by the International Association of Boards of Examiners in Optometry. 92 Pages. Paper Covers. $1.00. 1930.

This manual of Optometric Jurisprudence has been compiled from the writings of various men to serve as a ground work for the various Boards of Examiners in Optometry throughout Canada and the United States. Chapters 1, 2, 3, 6, 7, 8, 9, 12, 13 and 14 deal with optometric laws and their enforcement and Boards and their work. Chapter 4 by Dr. E. H. Silver of Washington, D. C., deals with visual standards in schools, industry, aviation and motor car driving. Chapters 5 and 11 deal with optometric education and model questions for State Board Examinations. Chapter 10 deals with the glazed goods question and what has been done to date to eradicate this evil. The book is nicely gotten together and contains much, that has hitherto been unpublished, of value in organized optometric work.

Among the other contributors, we find such well-known names as Chester H. Johnson, J. H. Brownlee, Leo M. Mayer, A. H. Nordland, E. H. Keikenapp, Howard C. Doane, G. J. Erskine, J. Fred Andreae, Emil H. Arnold and W. M. Kinney.

Dr. Lawrence P. Folsom, optometrist of South Royalton, Vermont, from whom this work can be gotten, has performed a work of decided merit which should serve as a guide for our State Board activities.

C. C. K.

ANNOTATIONS AND SOCIETY PROCEEDINGS

The American Journal of Optometry is the Official Organ of the American Academy of Optometry, as well as the Organ of the Arkansas, Delaware, Indiana, Iowa, Maine, Maryland, Michigan, Minnesota, Nebraska, North Dakota, Oklahoma, Pennsylvania, South Dakota, Vermont and West Virginia Optometric Associations.

The following gentlemen have consented to supply items of interest from their respective sections, acting in the capacity of

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OPTOMETRY AND OCULAR PATHOLOGY

The most widely used argument, says Dr. J. O. Baxter, optometrist of New Bern, N. C., that the oculist has against Optometry is our alleged inability to recognize eye diseases and their relation to the body as a whole.

It may sound strange, therefore, to say, according to our experience and the written admission of editors of journals of Ophthalmology, that the diagnosis of ocular pathology is the weakest point of the training of the oculist himself. This is not strange when we consider the usual amount of training the average self-appointed oculist has in his line of work. The present training of the optometrist in this phase of the work in his class A schools is superior to that received by the average oculist. Therefore, the modern graduate in optometry becomes of prime importance to the public and the family physician, because of superior knowledge in the recognition of eye disease and its relation to the organism.

Our work along this line becomes of much more importance if we are able to determine ocular pathology in its incipient stage and its etiology before destructive changes have occurred within the eye and other parts of the body.

Many organic lesions, says Dr. Baxter, and functional disturbances of almost any part of the body are apt to leave their sign in some manner, in the eye and its appendages. The proper interpretation of these signs could well be an obsession of the optometrist even to the point of intuition, which comes with the experience born of the light of midnight oil.

The optometrist may not deeply enjoy these ideals without the proper ocular and extra ocular aid and instruments made for this work; also a good working knowledge of diagnostic instruments that are also used by other major sciences, into which our field so often overlaps. I will name here, says Dr. Baxter, as essentials those aids that we have found by long experience to be of importance in ferreting out why we cannot make a comfortable pair of eyes by exhaustive optometrical correction. It seems almost superfluous to name a good ophthalmoscope, which is of use only for observation of the superficial nerve endings and other lesions within the eye.

The campimeter and perimeter gives information from these points to the entire extension of the optic nerve to the primary visual centers and calcarine fissure in the occiput. So it is seen that the ophthalmoscope is of value only in a relatively small area of a nerve, so vitally important in our work.

We may still further bear in mind that we can only see with the ophthalmoscope those lesions that are to be recognized, usually only after havoc and destruction have occurred, which are often irremediable, and that it leaves unobserved many functional disorders that could be treated and corrected before degenerative changes occurred, if we could have known about them at this time. It is at this stage that campimetry and perimetry often gives us this vital information.

Because some of us old timers have not enjoyed the benefits of a modern optometrical education is no reason why we should become a stumbling block to our profession, on account of not being able to recognize eye pathology. We still have textbooks and journals by which we may be enabled to know at a glance most any of the common eye diseases, if we have the instrumental means of seeing, measuring, charting these conditions. Your ophthalmoscopic diagnostic set with its light from a transilluminator driven through bone and other structures, penetratng into tissues that cannot be pierced without it, revealing to us pathologic changes and infections that so often brings optometry into disrepute and ridicule, if overlooked, and rightly so, for optometry must be an accurate profession, if it is to endure. It can hardly be called accurate if we "glass" a pair of asthenopic eyes that are suffering from infection or disease, in perhaps some remote part of the body, that may destroy sight, health or life itself, unless the patient happens to fall into the hands of an accurate practitioner before destruction of the tissues involved occurs.

How may we chart lesions of the central nervous system that are blocking or destroying the visual pathway by hemorrhage, tumor, gummata, atrophy, sclerosis, or what not, unless we have the equipment with which to work?

It is here that optometry becomes of the first magnitude of power and importance. We may send a patient to the dentist, saying to him, "My patient has an impending arteries sclerosis, also a first molar that transillumination shows to be cloudy. In this man-

ner we, as diagnosticians, dominate the situation. Optometry, continues Dr. Baxter, may easily, at the very beginning of these morbid changes, find them within the eye, and prepare the patient and physician to correct its cause before the arteries become rigid and hardened with the resultant high blood pressure and damage to the heart, etc. Today this and much more is what is being routinely done by the modern optometrist, the optometrist who on every hand is dominating the optometric field.

A. O. A. MEETS AT BOSTON

Drs. Fitch, Sheard, Leary, Givler, Luckiesh, Moss, Garfin, Needles and Neumiller were the speakers at the recent meeting of the A. O. A. held in Boston. The newly elected officers are Dr. J. F. Andreae, President; Dr. T. McBurnie and Dr. W. Kinney, Vice-Presidents; Dr. E. Kiekenapp, Secretary, and Dr. F. Stengel, Treasurer. Drs. W. Kimball, E. B. Alexander and G. Houghton were elected as Trustees. San Antonio was chosen as the meeting place for 1931.

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TRI-STATE OPTOMETRIC ASSOCIATION

Optometrists of Tennessee, Mississippi and Arkansas will hold a joint meeting at Memphis July 28-30 at the Elks Hotel. The chief speakers will be Drs. Copeland, Neumiller, Shepard and Mendelsohn. There will be no registration fee.

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NORTH DAKOTA OPTOMETRIC ASSOCIATION

The annual convention of the North Dakota Optometric Association will be held in Bismarck August 4th and 5th. Dr. H. Smith will be the chief speaker.

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WEST VIRGINIA STATE ASSOCIATION CONVENTION

The 23rd Annual Meeting of this Society was held in Fairmont on May 12th and 13th. The speakers were Dr. L. A. Davis, Dr. W. J. Boydston, Dr. C. Shepard and Dr. W. A. Mendelsohn. The newly elected officers are, according to Dr. O. E. Bell: Dr. W. W. Brown, President; Dr. J. Collins and Dr. O. E. Bell, Vice-Presidents; Dr. D. L. Gallagher, Secretary, and Dr. M. M. Mead, Treasurer. The Executive Committee is composed of Dr. E. C. Beaty, chairman; Dr. C. C. Wilson and Dr. L. E. Farnsworth.

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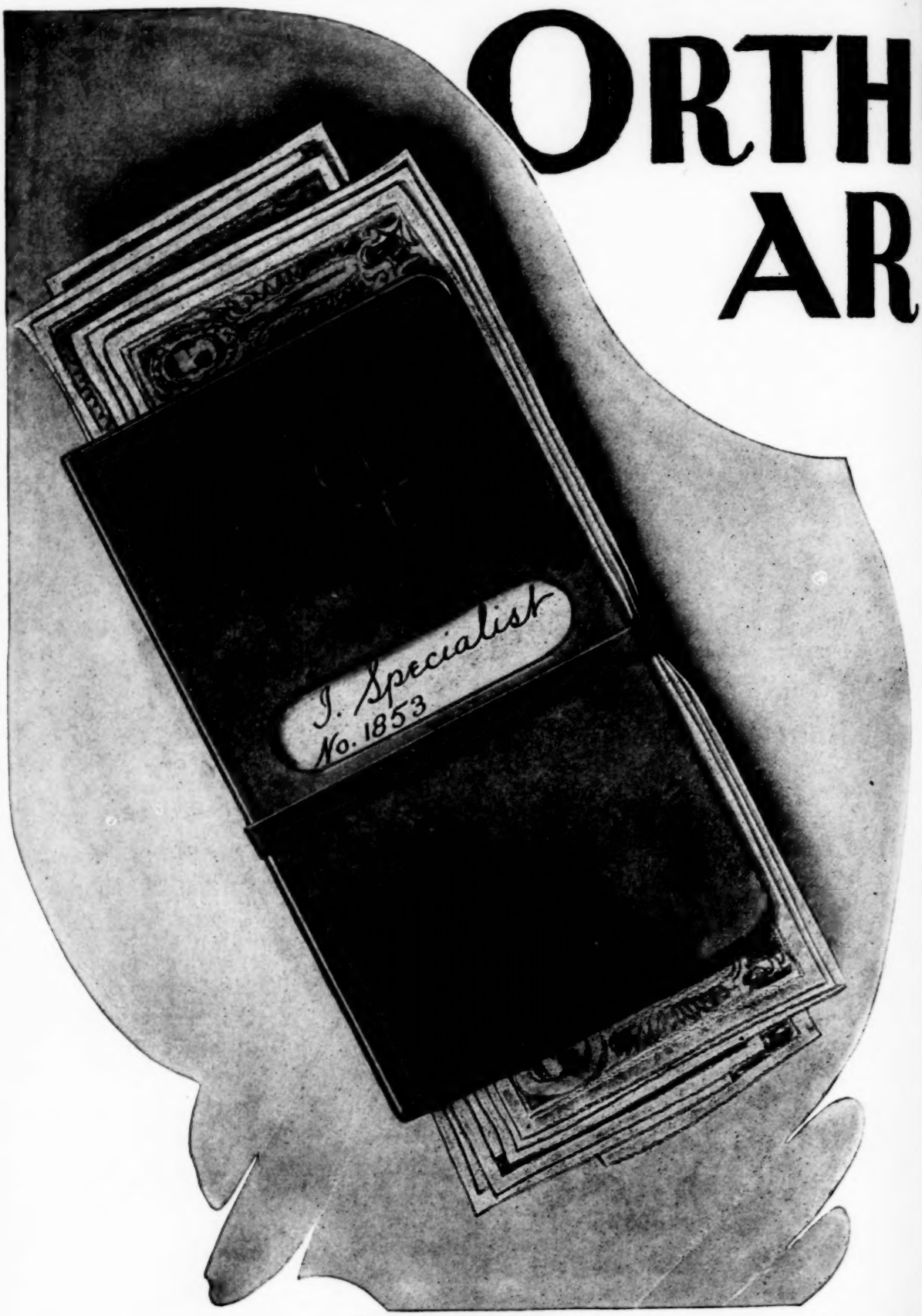

In Soft-Lites Too